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GENERAL REVIEWS AND SUMMARIES

VOLUNTARY PHENOMENA — EXPERIMENTAL

BY PROFESSOR R. S. WOODWORTH
Columbia University

Methods of Investigating Voluntary Action.—Three different means of bringing volition within the range of experimental observation have been introduced within the past year or two. Ach (1) interposes obstacles to the execution of an intended act, and so arouses an energetic will to perform it. Michotte and Prüm (2) require a choice to be made between alternative acts. Rowe (4), like Ach, selects for study an act that is difficult of execution, but the difficulty here arises from the unfamiliarity of the act, the method being properly described as genetic. Since a new field promises thus to be opened up for experimental study, a consideration of the methods is of first importance.

Each of these authors employs a combined introspective-objective method: after some brief act, the time of which is measured, the subject applies himself straightway to an introspective report. In the work of Ach and of Michotte and Prüm the introspection is what Ach calls 'systematic,' meaning complete, not directed towards any special point more than another. In spite of Wundt's scepticism regarding such introspection after the fact, the impression gained from the considerable mass of studies that have now followed this general method is that such observations have a fair degree of reliability. Certainly it is a mark of progress that generalizations vaguely precipitated from past experience should give way to induction from specific observations, even if the method of observation is subject to some difficulty.

These studies, like many similar ones on intellectual processes,

make use principally of the introspective data, relegating the objective measures to a distinctly secondary place. Statistical treatment of the objective measures is hindered by the excessive slowness with which they accumulate. Though the chronoscope may show that the process occupied but two seconds, the introspective review and record is likely to consume a quarter of an hour. The great benefit of combining subjective and objective observations, which would certainly seem to be the opportunity of *correlating* the two sets of data, will hardly be realized till the introspection becomes less 'systematic' and more sharply directed. Much more successful in this respect are the studies of Book[1] and Ruger.[2]

Ach's procedure is as follows. He first has his subject learn lists of nonsense syllables in trochaic rhythm, till the second syllable of each measure is strongly associated to the first. Then he presents the accented syllables, but, instead of requiring the reproduction of the associated syllable, instructs the subject to respond to each presented syllable by a syllable riming with it. The previously associated syllable tends to be spoken, and sometimes is not repressed in time to prevent a false reaction. After a few experiences with this procedure, and especially after a false reaction, the subject is led to a vigorous resolve to do what he intends and not to be led astray.

Selz (5), in a searching criticism, shows that certain unnecessary complications in Ach's procedure expose his conclusions to considerable doubt. He further raises the broad objection that Ach's method does not bring under observation the most characteristic of voluntary processes, namely the decision or choice, for the subject has decided on his general course of action beforehand, in accepting the instructions of the experimenter.

Precisely the choice between alternatives is the study of Michotte and Prüm, and their work is, as they themselves observe, complementary to that of Ach. Using the general technique of the reaction experiment, they showed the subject two numbers, with the instruction to choose between adding and subtracting them, or, in other cases, between multiplication and division. "Decide for serious motives, and then react," *i. e.*, press the key. The execution of the decision, the actual addition, etc., was not carried out, since the difficulty of this operation was apt to make it impossible to recollect the process of choice. But it was found that the process did not seem to the subject one of selection without action, for the reacting move-

[1] *The Psychology of Skill*, 1908.
[2] *The Psychology of Efficiency* 1910.

ment of the finger appeared to him as the execution of his decision, like the giving of an order or the starting of a machine. Though it might be doubted whether 'serious motives' could intervene in so trivial a context, yet it was found that the subjects took their task with all seriousness, and experienced a definite feeling of activity in the choice and accompanying reaction.

The relation between this procedure and that of Ach is well pointed out in a supplementary note by Michotte (3). With Ach, the voluntary act preceded the stimulus ("I *will* respond to this coming syllable according to instructions"); with Michotte and Prüm the voluntary act fell after the stimulus. Partly on this account, perhaps, the reaction times average considerably longer in the latter study. With Ach, the volition is not immediately executed, but gives rise to a 'determining tendency,' operative after the stimulus; with Michotte and Prüm the choice issued instantly in a (symbolic) execution. With the latter, the choice was attended with difficulties and was voluntary; with the former, the choice was previously made, but difficulty attended the execution. In both cases the muscular tension attending the performance was resolved at the moment of execution; it was not terminated, in Ach's experiment, by the act of will, but only by the execution. The muscular tension (and feeling of effort), accordingly, seems correlated not specifically with volition but with readiness to act under difficulties.

Rowe worked more specifically with voluntary movement, seeking the 'cue' of such a movement. The movement studied should be unpractised, and with adult subjects this condition can be met by an unfamiliar *combination* of movements. Movements similar to those of typewriting were used; a keyboard was prepared by fixing ten small rubber bulbs to a board to fit the fingers, and to each bulb was assigned a letter by means of a visible key. Out of the ten letters was formed a short list of words, and this list was written repeatedly till the writing became automatic. Other experiments were made with handwriting, upside down and in other novel ways.

Results.—The wealth of observation and high level of discussion in all these papers make it impossible to do any sort of justice to them in brief compass. A few of Rowe's results, as bearing more directly on movement may be mentioned. In executing an unfamiliar movement, the 'cue' and control are perceptional, not ideational; attention was fixed almost exclusively on the 'copy,' the key and the bulbs — *i. e.*, on visual objects. Kinesthetic sensations were for the most part only marginally present, and imagery of the

movement to be made, in either 'resident' or 'remote' terms, was absent. With practice, the perceptual control dropped from the focus of consciousness, and its place was taken for a time, in some subjects, by memory images of copy, key, etc.; but with continued practice these images faded and attention was occupied with a grasp of the total situation, "an organized 'set' of consciousness in which the details no longer require attention in themselves but only as parts of a complex whole." The 'set' needs to be voluntarily maintained, and this is the sole remaining voluntary feature of the performance. Introspectively the 'set' appears as a blend of organic, tactile-kinesthetic and visual sensations.

The author concludes that the rôle of images, and likewise the rôle of kinesthetic sensations, in voluntary movement have been greatly overestimated.

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VOLITION AND MOTOR CONSCIOUSNESS—THEORY

BY PROFESSOR EDMUND B. DELABARRE

Brown University

The appearance of an English translation of Bergson's *Essai sur les données immédiates de la conscience* (2) makes fitting an attempt to estimate the value of the seemingly new conceptions that he introduces into the treatment of the psychological problem of freedom. He distinguishes two kinds of multiplicity. The multiplicity of conscious states is not a discrete multiplicity, like that of number, but a qualitative multiplicity devoid of quantity. Thus the intensity of a simple psychic state is not quantity, but its qualitative sign. So also pure duration is the form which the succession of our conscious states assumes when our ego lets itself *live;* a qualitative multiplicity, with no likeness to number, and not measurable unless symbolically represented in space; an organic evolution which is yet not an increasing quantity; a duration whose heterogeneous moments permeate one another, forming an organic whole. But beset by the idea of space, we introduce it unwittingly into our feeling of pure

succession; we set our states of consciousness side by side in such a way as to perceive them simultaneously, no longer in one another, but alongside one another. The abstract homogeneous duration thus conceived is only the extensive symbol of true duration. The fallacies of both determinism and indeterminism arise from regarding intensity as a magnitude, not a quality; and from substituting for the concrete reality or dynamic progress, which consciousness perceives, the material symbol of this progress when it has already reached its end, or, in other words, from applying the conceptual method to the act and its series of antecedents, *after* the act has been performed. Neglecting to eliminate duration from the outer world and space from the inner world, the one party has been led to deny freedom and the other to define it, and thereby, involuntarily, to deny it too. Freedom cannot be denied except on condition of identifying time with space; it cannot be defined except on condition of demanding that space should adequately represent time; it cannot be argued about in one sense or another except on condition of previously confusing succession and simultaneity. The inner states of the fundamental self are living things, constantly *becoming*, not amenable to measure, permeating one another, and their succession in duration has nothing in common with juxtaposition in homogeneous space. Hence the idea of necessary determination loses every shred of meaning, and the self grasped by consciousness is a free cause. This freedom can be formulated only as the relation of the concrete self to the act which it performs; and it occurs rarely, because the moments at which we grasp ourselves truly are rare. We cannot define it, just because we *are* free, and any attempt to analyze it unconsciously transforms the process into a thing and duration into extensity, puts the already done in place of the doing, stereotypes the activity and thus makes spontaneity settle down into inertia and freedom into necessity.

Bergson makes little of two concessions, better consideration of which, however, may rescue us from confusion, leave us still in possession of our deterministic science and still allow us to assign a definite value to Bergson's interpretations. If causality is taken as meaning, not necessary determination, but only regular succession, he says (p. 202), "we should willingly grant these philosophers that their principle is derived from experience, but it would no longer prove anything against our freedom." Again (p. 221), he asserts that time can be adequately represented by space [that is, in the abstract homogeneous manner] 'if you are dealing with time flown,'

but not 'if you speak of time flowing.' These are all the concessions that a valid deterministic science asks. Consciousness as actually lived, duration as directly perceived, freedom as really experienced, may be such as Bergson unfolds at length; and though he is not the first to have so insisted, he has performed a definite service in emphasizing this their living and flowing reality. But science, though it cannot live but must conceptually describe reality, substituting therefor 'an artificial reconstruction, a patchwork of dead fragments,' is necessary and inexpressibly useful. Facts permit and expedience requires that it be deterministic, if its determinism implies, as alone it justifiably may, only 'regular succession' and not necessary compulsion. And this holds for psychical as well as for physical phenomena, even though Bergson has justly argued that to completely foresee an action would be to be actually performing it, and that the same inner causes never recur in their actual wholeness and produce the same effects a second time on the stage of consciousness. In brief, for the concrete, self-permeating, heterogeneous, indivisible and unmeasurable, organically whole life of actual flowing experience, there is the practical but indefinable freedom of Bergson; for science, which we must use to conceptually describe this living experience, a determinism which does not deny freedom in any but the indeterministic sense, holds true.

Miss Wentscher (3) has given an excellent brief description and analysis of the phenomena of volition. Her point of view is that of deterministic science. Covering the entire field, her treatment is too complicated to summarize fully. Her different chapters deal with the apparent nature of will, as not only foreseeing, but also helping to determine; with reflex and impulsive activities; with the motivation of volition, both conscious and unconscious; with an analysis of the act of will, finding in it not a particular element of consciousness, but a complex of other conscious elements; with the development of will in children; with various consequences of the nature of will; with the question of voluntarily directed thoughts, and that of moral conflicts; with the problem of the energy of the will, and that of its freedom; with a critical examination of other theories of will than that which she advocates, including those of Ebbinghaus, Münsterberg, Spencer, Lipps, Maier, James, Meumann; with the results of experimental investigation; and with a concluding summary of the whole argument.

In the analysis of will, Miss Wentscher's particular merit consists in recognizing that it is not a special psychical element or power, but

a peculiar synthesis of ideation and feeling; that it involves not merely anticipation of the end or a mechanical result of the alternation and strife of motives, but also a determination of varying strength, and a complex of approval and encouragement of some features, disapproval and suppression of other features, based on the nature of the whole personality under guidance of its ethical norms existent rather as emotional worth-values than as logical judgment-factors. Her defence of determinism emphasizes rightly the fact that indeterminists merely insist on this factor of approval and encouragement of motives, but refuse to analyze the conditions on which it rests or to recognize that it has definite conditions; and shows, further, not only that it must have definite conditions in the total personality, but also that responsibility and the justification of punishment can have no other than a deterministic basis. Herein she agrees with Fullerton and others who have possessed clear insight into this phase of the problem.

Ach (1) has contributed an unquestionably valuable analysis of certain phases of voluntary activity. But he himself makes it clear that he is not here investigating the real problem of will, namely under what conditions of outer and inner compulsion (or, we must add, of regularity without compulsion) an effective will-impulse arises, but only of how, on the dynamic side, an accepted will-impulse passes into execution. There can be no question that he adds much to our knowledge of the definite details of this process.

It would be interesting to set forth in detail the factors described by Ach as entering into the execution of an act already willed, when the moment for its execution arrives; how obstacles to its execution are met, and how the success with which they are overcome serves as a measure of the strength and effectiveness of the determination; how the details differ in individuals of different temperaments, under the influence of different attitudes and feelings, and in cases which are extended, complete or abbreviated, immediate, delayed, or in course of execution, successes or failures; and to follow him again in his acute analyses of the later consequences of success or failure. But we can only mention that he treats these matters at length. As especially important from the theoretical point of view, it seems desirable to emphasize: first, that he holds that these details can never be given by the observer as matters of self-observation during the experience, but only as 'perseveriende Inhalte' after the experiment is concluded; and second, that much that is present can never be described more definitely than as a 'Valenz,' a 'Bewusstheit'

rather than a 'Bewusstseinslage,' an 'unanschaulich gegebenes Wissen.' In other words, the experimental-introspective inquiry always meets inevitably with later and thus possibly changed representatives of former actual experiences, and with unanalyzable and indescribable forms of experience which may turn out to be complex and describable, never for direct experimental examination, but only for theoretical evaluation. If this be true, the experimental inquiry, though unquestionably it is to be encouraged as adding indefinitely to our knowledge of detail, can never hope to solve completely the problem of the execution of an act of will. When it comes to the question as to the motivation of the will, the causeful or causeless origin of decisions in general, which Ach promises to deal with in a later investigation, it is very sure both that the experimental dealing with the problem will add much of value to knowledge of the details of the process, and will also miss inevitably the solution of the question as to its fundamental nature.

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VISUAL MOTOR FUNCTIONS

BY PROFESSOR RAYMOND DODGE
Wesleyan University

It is suggestive that all the titles received by the reviewer are German. The coincidence need not be taken too seriously, however. American titles are plentiful enough outside the time limits of our review, and we happen to know of important work still incomplete. But this much is clear: psychological interest in the facts of eye movement, as contrasted with their 'introspective mythology,' as well as interest in the technique of objective methods for studying them, is far more general than it was a year or two ago.

In the papers which are chiefly medical, and which therefore may not be reported here,[1] Koch's work from the Kiel Laboratory is the only photographic study of the eye movements which is generally known and quoted. It is consequently variously unfortunate that, while he used the fairly satisfactory method of photographing the

[1] KOCH. *Arch. f. d. ges. Psychol.*, 1908, 13, 196–253.

corneal reflection, he neglected just enough of the technique to make his records misleading. The errors arise from the fact that vertical aberrations of fixation appear in his records as time.

A new comprehensive study of the angle velocity of the eye movements with present improved technique is badly needed.

Two studies come from the laboratory of von Kries. In the first Klara Grim (4) reattacked the problem of the threshold kinesthetic perception of eye movement. Inexcusably ignoring our present knowledge of visual fixation and the pursuit movements of the eyes, she holds with the Aubert tradition that the threshold in the perception of motion of a luminous point in a darkened room is the threshold of the kinesthetic perception of eye movement. The disturbing influence of autokinetic factors, and the secondary criteria given by long rectilinear excursions, are both cleverly minimized by confining the motion of the luminous fixation point to a circular path. This is undoubtedly a real advance in the technique. It is alleged to have the advantage of emphasizing the consciousness of eye movement through the continuous change in the relative tension of the eye muscles consequent to the circular pursuit. Independent of the questionable implications of kinesthetic threshold, the results viewed merely as a study of the threshold of the perception of motion are highly suggestive. She generalizes as follows: The smaller the radius of curvature of the path of the moving point the lower the threshold. Dark adaptation raises the threshold. Further ignoring the available data on the accuracy of fixation, and holding that the accuracy of eye movement is measured by the ability to count lines and dots close together, the author found the thresholds for horizontal movements between 9′ 32″ and 8′ 2″. It varies slightly for vertical and for oblique movements. The conclusion that (not only relative to its extent, but also absolutely) the accuracy of eye movement increases with the extent of movement might well have led to suspicion of the assumptions on which the evaluation of the experiments was based.

The second paper from Von Kries's laboratory by Marx (Leyden) and Trendelenburg (6) describes what is undoubtedly the most sensitive photographic technique for studying the fixation movements of the eye that has yet been published. A delicate mirror, attached to the cocainized cornea by an aluminum capsule, threw the image of a brilliantly illuminated slit on a suitable photographic recording device. A second mirror, fixed to the bridge of the nose, registered simultaneous head movements. In unenlarged records 1′ of eye

movement gave approximately 1 mm. displacement. The records show two kinds of intra-fixation movements. Minute tremors of approximately 1′, several times a second, are superimposed on longer irregular displacements of from 5′–8′, in 10″ of time. Involuntary head movements, within the limits of the head holder, also consist of tremors and longer displacements. But the long displacements of the head line are quite different from those of the eyes and do not coincide with them in time. The excellence of the technique, and the probability that the results will be quoted as the final authority on fixation movements, warrant a more extended critique. The author's assumption that eyes without the capsule would have less intra-fixation displacement is without any experimental basis. It is directly opposed by the recorded intra-fixation movements of other investigators. The theoretical exploitation of minimal values seems to be a questionable procedure. In view of the proven importance of extra-foveal factors in fixation, it seems absurd to assume that a black dot on a strip of white paper in a complex visual field constitutes a simple punctiform fixation object. Inspection of the published records shows pulse movements of the head in every head curve as well as in the one where it is specifically noted. The authors' assumption that this is capillary pulse is unwarranted. The angles of the curve seem altogether too sharp. The curves correspond better with the head movements consequent to the thrust of the big arteries as described in my experimental study of fixation. The misstatement that my technique shows an error of 10′ and is therefore not competent to show intra-fixation movements, is based on careless reading which would have been impossible if the authors had taken the pains to note the extent of the intra-fixation movements actually recorded by me.

Inouye (5), under the inspiration of Hering, measured the duration of positive and negative convergence movements by their apparent coincidence with the oscillations of a pendulum of variable length. The technique was exacting. Inouye was the only observer. Reliable results were found to be limited to minimal values. Movements of from 2°–12° took from 180σ–260σ. Negative convergence movements were found to be slower than positive.

An excellent device of Bárány (1) for measuring the torsion depends on the microscopic examination of the iris markings.

Bartels' regulation of the position of the eyes by the ear (2) is an exhaustive study with original experiments. The arrangement is by animal species.

Buys (3) describes a tambour, air transmission, recording device which is operated by the movements of the cornea, under the lid.

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RIGHT- AND LEFT-HANDEDNESS

BY PROFESSOR ERNEST JONES

University of Toronto

Lewandowsky (3) reports the following interesting case, which has an important bearing on the theory of the functional difference between the two cerebral hemispheres. A patient had a left-sided paralysis and other symptoms that pointed to a lesion in the right side of the brain, and at the autopsy a tumor was in fact found in the right centrum ovale. The patient also presented an aphasia and a right-sided apraxia. Such cases have been previously described in left-handed people, the normal functions of the right and left halves of the brain being evidently reversed. What was peculiar in the present instance, however, was the fact that the patient was throughout *right-handed*. A few previous cases have been reported in which a right-sided lesion in a right-handed person had given rise to aphasia, though the possibility has not received general credence, but this is the first case in which such a condition has caused both aphasia and apraxia; to such cases the name of 'crossed aphasia' or 'crossed apraxia' is applied. From this careful description it would seem that in future the possibility must be accepted of the speech and purposeful movements being localized in the right hemisphere even although the person is right-handed.

Klehmet (2) details the rare occurrence of an acute onset of left-handedness. The patient, a man of 21, was suffering from a catatonic hebephrenia engrafted on to a congenital imbecility of a light grade. Synchronously with the outbreak of the hebephrenic symptoms a total left-handedness appeared, for the first time in the patient's life; that he had previously been throughout right-handed was established beyond all question. The left-handedness extended to the carrying out of such difficult matters as bread-cutting and writing, the use of the left side was preferred throughout, and the patient frequently confounded the two sides of the body. The reviewer has no doubt that the occurrence has nothing to do with the usual left-handedness, but was purely psychogenic in origin and nature; he would further surmise that the case was one showing the dyschiric syndrome previously described by him—*i. e.*, it was a question of an hysterical symptom appearing in catatonia—in which the sudden and complete transference of the functions of one side of the body to the other is a well-established occurrence.

Bauer (1) publishes a very detailed study on the subject of the capacity to estimate weights with either hand. From his summary of conclusions the following may be abstracted. In general right-handed persons overestimate weights with the left hand, and left-handed persons those with the right. Overestimating of weights with the right hand may be an evidence of 'latent' left-handedness. The error in estimation, which even by normal people may be very considerable, is not due to any difference in the muscular power of the two sides, but to a difference in the 'Schwereempfindung' itself; it bears no relation to age, sex, or intelligence. The evidence collected speaks in favor of the 'Schwereempfindung' being a special sensation; it may be disturbed quite apart from any troubles in deep sensation (clinical sense of the term). Hypotonia leads to an error of overestimation, hypertonia to one of underestimation.

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REACTION TIMES

BY DR. HERBERT WOODROW
University of Minnesota

Recent studies of reaction time, while they have brought forth no startling results, show well the great variety of the problems that may be attacked by this method. At the same time, they emphasize the fact that accurate results can not be obtained without careful attention to the technique of the method.

Dunlap and Wells (3) reopen the old question of the effect of intensity of stimulus on reaction time. "That differences of intensity, as such, within limits of what may be called satisfactory intensities, may cause differences of reaction time, is not established." They also attack the problem of why simple reaction times are commonly shorter for auditory stimuli than for visual. An answer to this problem is not reached; and it seems to the reviewer that the problem itself is not quite definite enough, in view of the fact that Wundt finds that the reaction times for sound and for light are about the same when minimal stimuli are used, and the further fact that a marked effect of both intensity and duration on reaction time has been found for light, and, generally, also for sound.

In this connection may be mentioned the results of Minneman (7) on the rapidity of perception of sound and light stimuli. Minneman finds that increase in intensity shortens the time of perception of both visual and auditory stimuli, but that it depends upon special circumstances whether sound stimuli or light stimuli are perceived more rapidly, so that one can not say in general that either mode of stimulus furnishes the smaller times. Dunlap and Wells did not measure the intensity of their stimuli, but state, with respect to their sound stimulus, that the sound was made decidedly loud, but not disagreeably so. The visual stimulus was the illumination of a white screen by the flash of a helium tube. They found in the case of simple sensory reactions that the visual reactions were longer than the auditory, but that when both the sound and the flash were presented simultaneously, with reaction to the flash—the attention being directed exclusively to the flash in the preparatory interval—the reactions were practically as quick as the simple sound reactions. That the comparatively short reaction time to the flash, when the flash occurred simultaneously with a loud sound, might indicate that the reaction was really to the sound rather than to the flash, is a natural explanation, suggested by the

authors, but not accepted by them. They refrain from offering an interpretation, until after further work.

In another article (4), Dunlap studies "Reactions to Rhythmic Stimuli with Attempt to Synchronize." By rhythmic stimuli are meant regularly recurring stimuli. The rate of succession of the stimuli, which were sometimes visual and sometimes auditory, varied from about ¼ to 2¼ secs. The reactions consisted in attempts to beat upon a practically noiseless key in time with the stimuli. As soon as the reactor began to receive stimulations, he prepared to react, beginning his reactions as soon as he caught the tempo. The recording, which was done graphically, began after about a dozen reactions. As regards the constant errors in the subject's attempts to synchronize his taps with the stimuli, they are found to be, as Cattell has remarked is so often the case with constant errors, extremely variable both in magnitude and direction. They show no very definite relation to the rate of the stimuli. As regards the mean variations, they are characterized as large, and found to be independent of the constant error, being as large, on the whole, when the constant error is small as when it is large. They are also as great, on the whole, at the faster rates as at the slower, thus making the short intervals relatively more irregular than the longer. The author urges the general similarity of this kind of synchronizing reaction and 'the characteristic process' in the complication experiment.

Henmon (6) reports some 'preliminary' results on "The Relation of the Time of a Judgment to Its Accuracy." The subjects had to judge concerning the relative length of two lines, 20 and 20.3 mm. long. The time elapsing from the presentation of the lines to the completion of the judgment (the latter indicated by a movement of the hand) was measured by the Hipp chronoscope. Immediately after each judgment, the subject assigned, in terms of a scale of four degrees, the amount of confidence felt in the accuracy of the judgment. A number of interesting conclusions are drawn, but even under the conditions of this particular research, the relation of the time of a judgment to its accuracy appears somewhat complex, so that further work, expressly outlined by the author, is needed before an unequivocal statement can be made.

From Külpe's laboratory comes a study by Westphal (11) of the structure of the subordination (Unterordnung) consciousness, that is, of the consciousness of persons busied with the simultaneous solution of 'Haupt- und Nebenaufgaben.' Thus, after a signal, the subject was shown an irregular polygon, and instructed to deter-

mine, as principal problem, the number of angles, and as subordinate problem, the longest side. Upon solving both problems, the subject released a key, and the time between exposure of the polygon and the release of the key was recorded by a chronoscope. The reaction times obtained in this manner are intended as a control upon the very extensive and detailed introspections of the subjects; but their value in this respect proves so slight, that one gets the impression that their measurement is itself merely a 'Nebenaufgabe,' carried for whatever may be got out of it. One interesting conclusion from the reaction times is, that, when the difficulty of the principal task is increased, more time is given its solution and there is no decrease in the certainty of the result, but if the task is a secondary one, and then greatly increased in difficulty, it usually suffers from neglect, so that there is no increase in reaction time, but, instead, more uncertainty and a greater number of incorrect answers. Moreover, the total time required for solution of both problems is usually shorter when the harder of the two is raised to the rank of principal problem than when it is carried merely as a side problem. The author urges the agreement of these conclusions with every-day experience. Reaction times for a purely synthetic method of solving the problems were found to be longer than for a purely analytical method. It was also found that it makes an immense difference whether the subject is told to react as quickly as possible or not, both in the time and the method the subject chooses for solution of the problems.

Wells, in a study on free association time (**9**), concludes that longer times, while apt to be the result of emotional coloring, may also be due to suppression, distraction, indecision, and the like. He finds the effect of practice (**10**) to be a decrease in the association time to a limit approximating 1.2 sec. At the beginning of practice, the subject may be any distance from this limit up to 3.0 secs. or more.

D'Allonnes (**1**) contributes an interesting criticism of the reaction time method as a clinical procedure in connection with the measurement of attention. He has devised for clinical purposes a form of reaction experiment not hitherto used, which, he believes, is free from objections that are to be made to the clinical use of simple reaction times, namely, too much delicate apparatus, the danger of automatic reactions, and needless precision in the time measurements.

Angell (**2**) and Dunlap (**5**) each have articles dealing exclusively with the technique of the reaction method. Angell presents data which confirm the conclusions of Breitwieser concerning the great decrease in simple reaction times that occurs with decrease in the resistance of the reaction key.

Salow (8) in an historical essay, traces in considerable detail the development of the reaction experiment, beginning with Bessel (1822). He refers to about one hundred and forty of the experimental studies made in this field since 1860.

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FATIGUE

BY DR. F. L. WELLS

McLean Hospital, Waverley, Mass.

In spite of the obvious difficulties that beset such an inquiry, the question of the blood supply of the working muscle has not suffered neglect. Geissler and Zybell (1) report a series of experiments especially directed towards the question of the volume of the blood vessels under these conditions. The plethysmograph can scarcely be applied to the moving member, for, aside from the inherent difficulty of obtaining accurate readings under these circumstances, comes the consideration of whether the volume changes are the product of vascular ones, or of alterations in the distribution of the body fluids. The authors therefore employed the method of im-

mersing one arm of the sitting subject in a water plethysmograph with recording attachments, while the other arm executed a series of contractions upon a rubber bulb filled with water, continually emptying and refilling the same; the rate of the contractions varying from 30 per minute at the beginning to 20 at the end of an observation, which ordinarily occupied four minutes. Coincidently with the beginning of work is reported a fall in the plethysmograph curve, which may proceed throughout the four minutes, or be interrupted by a transient rise during the second minute. The pulse beats also become smaller. These changes are rapidly obliterated as soon as the work ceases, the normal balance being quite restored after some three minutes. Through continued experimentation the amount of fall in the curve is reduced to a small fraction of its original proportions. The finding of this negative change in the plethysmograph curve is, as the authors point out, contrary to the ordinary views upon the matter, which suppose a widening of the vessels to allow an increased blood supply to the working muscle. The authors consider it more probable, however, that the blood supply is proportional in the two arms, and that the resting arm gives a proper criterion for the working one. A psychological interpretation is offered, based upon observations of the relation of blood pressure to the intensity of voluntary effort. This, rather than the absolute quantity of work, determines the amount of blood pressure. The fall in blood pressure here observed is, then, to be taken as an expression of the *Unlustgefühl* that comes with fatigue; and its disappearance with practice goes hand in hand with the lessening of fatigue under these conditions.

In a later contribution to the same journal, these results are subjected to a searching criticism from the pen of E. Weber (6). The apparently simple and really very difficult method of the plethysmograph has resulted in a number of far from unimpeachable investigations that have tended to discredit the method among many. The use of a symmetrical member is strongly contraindicated, on account of the slight sympathetic movements that it induces. Relatively unrelated muscle groups should be employed; thus Weber has made use of an isolated movement of the foot and toes. Under these conditions is observed an increase of the volume of the arm, and not a decrease as above reported. Further objection is raised on the score of insufficient control of the breathing. It is not sufficient simply to endeavor to breathe regularly; without the actual pneumograph record such results can never be conclusive. In respect

to the origin of the phenomena, Weber's ideas are not so entirely different from those of Geissler and Zybell, for he also assigns to them a psychological interpretation, *i. e.*, that they are determined by the presence of motor imagery. In a hypnotized subject the possibility of actual movement was excluded, and vivid motor imagery of various limbs was suggested. There was observed an increase in the volume of the limb involved. Autosuggested imagery might have the same effect in the waking state; but passive movements, executed by another person, did not show the phenomena if the patient's attention were kept from them through hypnosis. Further, Weber speaks of simultaneous observations of the two arms, in which vivid motor imagery is suggested for one or the other arm of the hypnotized subject, motionless throughout. Ordinarily both arms increase in volume, the arm immediately involved more than the other; but if great emphasis is laid in the suggestion upon the quiescence of the other arm, it may undergo a slight decrease. Here Weber supposes the clue to the results of Geissler and Zybell. These workers, using one arm, would by all canons of plethysmographic method be compelled to be very careful not to move the other arm, which would be in the plethysmograph. As a result of this inhibition, vaso-constrictor impulses would reach this arm similar to those which Geissler and Zybell report. These inhibitory impulses are obviated by the use of asymmetrical portions of the body. Weber does, however, postulate that definite fatigue effects are shown in the plethysmograph curves, even to the extent of an objective indication of fatigue, so that unfatigued subjects alone are suitable for the conventional plethysmographic experiments.

The experiments of Imbert (**2**) are rather meagerly described, but the investigator appears to have made the observation that after continued (tetanic?) contraction of a group of muscles, brief relaxation brings about a very considerable degree of recovery. The phenomenon was demonstrated upon a number of subjects, including a group of soldiers, who showed some correspondence of endurance in this test with marching ability. He interprets the fatigue phenomena observed as due essentially to the reflex inhibitions from the pain involved. The observation regarding fatigue in tetanic contractions is not a novel one, and has been supposed to be due mainly to the exsanguination of the muscle as a result of its prolonged contraction; though some rôle, here as elsewhere, is doubtless played by the pain sensations.

Apropos of a six-day bicycle race in which drugging figured largely, especially, according to the author, among the American

participants, Loewy (3) prints a brief account of some experiments with reference to the combating of fatigue through pharmacological means. He is concerned especially with the consumption of oxygen and the production of carbon dioxide as associated with fatigue; seeking by chemical means to lessen the necessity for oxygen. The endeavor is to neutralize the acid products of metabolism, which earlier researches indicated to be practicable through the administration of spermin, a testicular preparation. The experiments reported are with two dogs, operating a sort of electric treadmill, the effect of work done under the influence of subcutaneous injections of spermin being compared with control experiments in which spermin was not administered. In the more extensive observations with the first dog, the relative consumption of oxygen is markedly diminished after the injection of spermin. The results with the second dog are less clear-cut though the author believes them confirmatory. He reviews also the work of Brown-Séquard which first suggested the dynamogenic effect of testicular substances, and which was confirmed by the observations of Zoth and of Pregl. They appear to have favored practice and recovery, and to have opposed fatigue. In the present experiments the spermin seems to have postponed the fatigue effects. The mechanism of its action may be twofold; it may chemically neutralize the acid products of fatigue metabolism, or it may greatly facilitate their removal along the normal channels.

In the mental multiplication of large numbers, Professor Thorndike (5) has developed an excellent method for the work-curve, in that so little portion of the experimental time is taken up with activities external to the test. It is limited in application, of course, by the fact that many individuals cannot perform the tasks at all. The present paper describes some of the properties of the work-curve obtained with this method, using three-place figures which the subject was allowed to see, and did not have to commit to memory. The results are scored in terms of time for a given number (4) of examples, with specific allowance for errors. Of the sixteen subjects, six worked from 3 to 8 hours continuously, the remainder 4 to 12 hours with pauses for meals. Then the work-curves thus obtained were compared with shorter performances on the following day. Three of the subjects seemed to do as well at the last of the work as after the rest period; these were also the three subjects whose total work periods had been shortest. The variability of this relationship is very great, the percentile figures ranging in the table from 61 to 297, though the different lengths of the work

periods make it inadvisable to fix a central tendency for the whole group. For a seven hours' work period, the ratio approximates 1.55. As the time of doing the work increases with fatigue, so do the number of errors; to cursory inspection it appears that rapid work in an individual has to some extent gone hand in hand with fewer errors, as often happens. For the cases available it is, however, quite certain that greater efficiency has been associated with resistance to fatigue; the subjects who do better fatigue less. If their efficiency be the result of practice, it would mean that in this function practice tends to reduce fatigability; the author does not apparently consider that it is so, but that the subjects may be taken as approximately equal in practice. Practice often overbalances fatigue for quite a distance through the work period. Allowing for the probable effects of practice, it appears that the cause of fatigue is a steady one, without sudden exacerbations. The question of the relation between the feelings of fatigue and objective fatigue loss is studied through introspective notes and receives its usual negative answer. The paper concludes with general tables giving the results by successive four examples for each subject.

While contributed only as a 'minor study,' the experiments of Norris, Twiss, and Washburn (4) deserve special mention from a methodological standpoint. They deal with the effects of fatigue on judgments of pleasantness and unpleasantness in color stimuli. "A state of fatigue," the account opens, "may naturally be expected to lessen the pleasantness of pleasant experiences and to increase the unpleasantness of unpleasant experiences." Very likely it is the feeling of fatigue that is here principally meant, though the actual condition also opens interesting possibilities. It was endeavored to observe how the judgment of color was affected by the subject's having to perform a long series of such judgments. The fatigue was presumably strictly confined to the mental work of the judgment, for any visual fatigue would obviously interfere much with the interpretation of the experiments. The ninety colors of the Bradley series were used as stimuli, and the subject was, upon presentation, asked to assign the affective value of the color to one of seven degrees in a pleasantness-unpleasantness scale. After judgments upon the ninety had been recorded, the first six colors were again presented, and it was endeavored to see whether the grades now assigned to these colors showed any constant deviation from the previous ones. There were thirty-five women subjects, college students with four exceptions. In seven cases the average position of the six colors

was higher for the second exposure than the first; for three it was the same, for twenty-three it was lower, with a range of .1 to 1.5 positions. There is thus indicated a slight downward tendency. Since the amounts of fatigue (*resp.* feelings thereof) are not uniform, and may even be negative, the small and variable differences do not reflect unfavorably upon the principle of the method. It is worthy of note that the changes in affective value observed are almost entirely confined to the shades and tints; the changes in the saturated colors are nearly balanced, though as frequent as in the others. No significant relation appeared between the tendency to change and the character of the judgment, though it might be shown in more elaborate experiments. The present ones are another demonstration of how readily many of the so-called *imponderabilia* of psychology lend themselves to a quantitative treatment, and the interesting character of the results to be anticipated from it. The brief study just described is replete with suggestions for further work.

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INSTINCT

BY PROFESSOR ROBERT M. YERKES

Harvard University

During the year 1910 the most important general discussion of this subject occurred in a symposium at a joint meeting of the Aristotelian and the British Psychological Societies. The distinctive points of the views set forth by the five participants in the symposium may be presented.

Charles S. Myers (4, 209) contends that instinct and intelligence are inseparable; that each in its subjective aspect is found in every act. He thus briefly states his case: "Instead of defining instincts

as 'complex reactions which are perfect the very first time,' I have endeavored to show that they are all within very variable limits improvable by practice or by imitation, or are modified by changed conditions of environment. Further, in place of the usual definition of instincts as merely 'fixed innate activities,' I have maintained that the essential mental concomitants of instincts are the feeling of activity and a vague consciousness of the behavior to be achieved. I would urge that the existence of these central factors proclaims the presence of intelligence throughout instinct, and that, as the organism becomes endowed with an increasingly large number of mutually incompatible modes of reaction, the intelligent aspect apparently comes more and more to the fore while the instinctive aspect apparently recedes *pari passu* into the background" (p. 214).

The pith of C. Lloyd Morgan's contribution to the discussion (3), aside from his specific criticisms of Myers' view, is found in a note at the end of his paper. He states, in effect, that the control system of intelligence and the experience termed intelligent become differentiated from the mechanism and consciousness of instinct. "The performance of an instinctive act so stimulates the centers of intelligent control as to afford the primary data of experience. . . . It is therefore essential to distinguish between two orders of heredity: first, that which obtains within the automatic system, and which thus determines the nature of the hereditary responses; secondly, that which obtains within the system of intelligent control, and which thus determines the hereditary likes and dislikes. . . . I regard the cerebral hemispheres as the differentiated control system and conceive that they play no functional part in the automatism of instinctive behavior" (p. 229).

Taking exception to the main contention of Myers, H. Wildon Carr (1) presents a conception of instinct in its relation to intelligence which is essentially that of the philosopher Bergson. Instinct and intelligence, he states, designate interpretations, not observable facts. They differ not only in degree, but still more importantly in kind, for they are strikingly different modes of apprehending reality. "But besides the intellect, and implied in our knowledge of its limitations, is a power of intuition, that is, of apprehending reality not limited by the intellectual categories, and this reality is the living activity itself apprehended as a real duration" (p. 236).

G. F. Stout (5) enters the discussion with the following condensed summary and confession of faith. "Three main questions emerge in the papers of Mr. Myers, Mr. Carr, and Mr. Morgan. (1) Is

instinct a special way of knowing or is it a merely biological fact? Mr. Carr, following Bergson, holds it to be a peculiar mode of cognition. Mr. Morgan and Mr. Myers concur in regarding it as essentially a form of biological adaptation and as being, in this respect, on a level with reflex action. On this point, I agree with them as against Mr. Carr. (2) Is every intelligent action also instinctively determined? Mr. Myers says yes; Mr. Morgan says no. I agree with Mr. Morgan. (3) Is every instinctive action as such also determined by intelligence? Mr. Morgan says no; Mr. Myers says yes. I agree with Myers, and my position is here more radical and uncompromising than his" (p. 237). Stout regards instinct as a substitute for the capacity to learn. To him it seems relatively unimportant in man.

The conception of instinct defended by William McDougall (2) is markedly similar to the views presented by Myers and by Stout, and as markedly dissimilar to those offered by Morgan and Carr. "A typical example, then, of a purely instinctive action, implies the existence in the creature's innate constitution of, first, a specialized perceptual disposition; secondly, a specific conative tendency that is excited when this perceptual disposition is played upon by the appropriate sense-impression; and thirdly, some coördinated system of motor channels through which the conative tendency works towards its satisfaction. The three things belong together; each implies the other two; each can subserve the life of the organism or of the species only in conjunction with the other two; all three together constitute a functional unit which is transmitted as such from generation to generation; and to such a functional unit of the innate constitution only, and to no part of it alone, and to no other fact or feature of the organic world, can, I submit, the name instinct be properly applied" (pp. 264–265).

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SPECIAL REVIEWS

APPLIED PSYCHOLOGY

Influencing Men in Business. The Psychology of Argument and Suggestion. W. D. Scott. New York: Ronald Press, 1911. Pp. 168.

It does not often fall to the lot of a psychologist to be read as eagerly and as seriously by the producers and distributors of goods as by the members of his own profession. This has come to be the case however with William James and his *Principles of Psychology.* No small credit for this fact is due to Professor Scott and his clearly written popular presentations of many of James's analyses. The present book of 168 pp. elaborates his analysis of the chief types of decision, illustrating them by examples drawn from business life. Along with this is an exposition of the ideo-motor character of suggestion. Typical business situations are pointed out in which argument or suggestion would be most likely to be effective in influencing men. Suggestions for making arguments and suggestions effective follow, illustrated by advertisements.

H. L. Hollingworth

Columbia University

THE STEPPING REFLEX

Remarks on the Reflex Mechanism of the Step. C. S. Sherrington. Brain, 1910, 33, 1–25.

As might be expected of a presidential address to the Neurological Section of the Royal Society of Medicine, this paper is concerned with certain general conclusions which the author has reached with regard to the mechanism of locomotion rather than with detailed experiments. Although practically all of the experimental evidence from which the author draws his inferences has been published previously by him and by others, the analysis of locomotion and the interpretation of the experimental findings is new. Analysis of the act of locomotion reveals two opposed sorts of reflexes; the one postural, attitudinal or static, the other kinetic. *Static reflexes* may be evoked by appropriate stimuli in cats and dogs in which the brain stem has been

transected between the anterior and posterior colliculi. The muscles by which the reflex is executed are the extensors of the fore and hind limbs, the erectors of the spine and the elevators of the lower jaw. The efferent portions of the reflex arc are the motor neurones for the muscles named. The afferent portion of the arc is neither from the skin nor from the otic labyrinth, but it is from the proprioceptive system of the extensor muscles themselves. When the static reflexes are aroused after such an operation as that described, there results the condition called by Sherrington, decerebrate rigidity. This decerebrate rigidity is nothing more than the uninterrupted action of the static reflexes which are normally present in all pedal locomotion. Its function is to maintain the *ususstatus* by opposing gravity. *The kinetic element* in locomotion depends upon the alternate action of extensor and flexor muscles. This *alternate contraction* of extensor and flexor muscles is achieved by *reciprocal innervation* and *reciprocal inhibition* of the two sets of antagonists, the seat of which is in the spinal cord.

Study of the decerebrate preparation shows that the kinetic element of the reflex is *rhythmic*. Faradization of an afferent nerve at a rate of 30 shocks per second produces a reflex response of 2 per second. This result shows that there is a *refractory period* during which arcs are inexcitable, a fact analogous to the refractory period of the heart. The normal stimulus to kinetic, as well as the normal stimulus to the static, reflexes is a proprioceptive stimulus resident in the muscle itself. Passive movement of a limb excites the reflex but not so effectively as an active contraction of the muscle. The relation between the extensor and flexor arcs is further studied in the phenomenon of *reversal* and the phenomenon of the *rebound*. The meaning of the reversal of a reflex is explained in the example which follows. The sartorius muscle is composed of two bands, a medial band and a lateral band. The medial band is inserted into the medial, posterior, surface of the tibia; the lateral band is inserted into the patella. The medial band therefore flexes the leg while the lateral band extends the leg. Passive extension of the leg causes reflex contraction of the flexor portion of the sartorius; passive flexion causes reflex contraction of the extensor portion. The phenomenon of the rebound is the exaltation of a reflex after its suppression by reciprocal inhibition. For example, the continued contraction of a flexor muscle inhibits the contraction of an extensor muscle. In decerebrate preparations, withdrawal of the stimulus producing flexion is followed by active extension. The rebound is due to a

post-inhibitory exaltation of activity in the previously inhibited extensor centers. Head observed a similar phenomenon in the diaphragm. Faradic excitation of the central end of the vagus which was just too weak to produce tetanus in the diaphragm was found to produce tetanic contractions of the diaphragm after the contraction of the diaphragm had been inhibited by inflation of the lungs. The locomotor reflexes which produce alternate postural and kinetic phases are self-regulative in a manner similar to the *Selbststeuerung* of the respiratory center, which was discovered by Hering and Breuer. This self-regulative quality is conferred upon the locomotor reflexes by the phenomena of reciprocal inhibition, the rhythmic response of the arcs by virtue of the refractory period, the alternate action of extension and flexion in virtue of the phenomenon of reversal and by the post-inhibitory exaltation of the reflexes.

H. C. Stevens

University of Washington

DISCUSSION

DR. YERKES' VIEW OF PSYCHICAL CAUSATION

Dr. Yerkes' *Introduction to Psychology*, which seems to be addressed to psychologists fully as much as to the novices in the subject, furnishes numerous grounds for profitable discussions of psychological theory. The most important of these battle-fields, it seems to me, is marked off by the topic of 'psychical causation.'

Dr. Yerkes' defense of the theory of psychical causality does not make clear just what he means by the theory. At least, I am puzzled by his statements and arguments. "There are two ways in which a psychological phenomenon may be explained," he says, "either in terms of other psychological phenomena, as one should naturally expect that it would be, or in terms of the physical conditions which accompany it. The former type of explanation is based upon psychical causation; the latter upon the correlation of psychical with physiological phenomena" (pp. 317–318) How shall a psychological phenomenon be explained in terms of other psychological phenomena? In the paragraph following that from which I have just quoted, Dr. Yerkes insists that the explanation is a matter of "knowledge of the circumstances or conditions under which a particular phenomenon appears," and that this knowledge "concerns relations of phenomena." Finally, if I have not misread, he limits the relations with which psychical causation is concerned to "the sequence of

mental events and the regularity thereof" (p. 319). He goes into details somewhat sparsely: "One image definitely and persistently leads to another; one idea to another; one sensation to another; a particular sensation is uniformly followed by a particular feeling, emotion, memory. Are not these instances of sequences of events similar to those which give the basis for causal explanation in physics? I think so."

If Dr. Yerkes had limited the application of the theory to associational sequences it would be intelligible, since all we know at present about these sequences is exhausted by the psychic facts. But Dr. Yerkes explicitly drags in the sequence of sensation and after-image. "How many hundred times have I observed that a certain visual sensation is followed by certain after-images?" he asks in this connection. It is this application of the doctrine of psychic causation which puts me completely at sea as to what it means, as well as to the nature of the distinction, in Dr. Yerkes' scheme, between the psychical and the physical.

Suppose we grant that the sequence of sensation and after-image is invariable, and is such that the law thereof may be stated passably well without reference to the structure and function of the eye. The sequence of sensations in the case of the flash and the roar of a cannon is just as invariable, and is just as capable of formulation without regard to other facts. In each case, the law of the sequence, if formulated without numerous reservations, is far from exact. Many flashes are not followed by roars, and in many cases a sensation of green is not followed by a pink after-image. Why, then, does Dr. Yerkes insist that the sequence of flash and roar is a physical relation, and that the sequence of sensation and after-image is a psychical relation?

If we can legitimately 'explain' the sequence of sensations and other contents of consciousness to which Dr. Yerkes refers in the statements I have quoted, without reference to anything other than the immediately experienced contents, why can we not 'explain' in the same way all of the millions of sequences in the world of phenomena — the sequence of color sensations when iron is raised in temperature; the sequence of tactual, muscular, visual, and auditory sensations when a gun is fired; etc.? Dr. Yerkes, be it noted, explicitly claims that all these phenomena are subject-matter for psychology.

I am not at all certain that Dr. Yerkes would not agree that these sequences are all capable of psychical explanation. If he were clearly committed upon this point the discussion would be much simplified.

But from the whole trend of his book I am forced to conclude that he would not and could not adopt this view. If it were adopted, there would be no point to the attempt to place psychical causation beside physical causation: psychology would be limited to the bald and inexact statement of observed sequences in the phenomenal world, and science would begin with the supplementation of the observed and observable facts by certain constructions which make the generalizations more exact and more readily handled. This is practically the position of Huxley in regard to the physical and non-physical ways of investigating the world.

The question of psychical causality rests ultimately on the question of the nature of physical causation. This, I think, no one will dispute. The main question of physical explanation is: Is it possible to explain causally without going beyond the observable facts? If it is not possible to give an explanation of the connection of the flash and the roar of the cannon which would be accepted as causal, without bringing in, explicitly or by implication, the logical symbols which are commonly referred to as *matter*, we may safely assume that there is a similar impossibility in the case of the connection of after-image and sensation. On the other hand, if it is possible to give a causal explanation in the first case without material reference, it is proper to consider the question whether that explanation is to be called 'physical' or 'psychical.'

The question of psychophysical causation, as distinguished from psychical causation, is also left in a somewhat confused state by Dr. Yerkes' treatment. There is never any serious dispute among psychologists as to what psychology includes; but the determination of what is psychical and what is not, is a treacherous task. It is regrettable that Dr. Yerkes does not set forth his principle of discrimination, especially since his application of the term 'psychological' is apparently more liberal than that of many of his fellow workers. Why, for example, does he refuse to assign a psychological character to the body? In what sense does he use the term 'body' and in what sense does he use the term 'mind'? These terms, unfortunately, have manifold meanings.

Certainly we can investigate the causal relations of sensations which we refer to external objects and sensations which we refer to the body, without stepping out of the parallelist's position. The flash of lightning and the start and exclamation of the frightened spectator; the perceived apple and the reaching forth of the hand to take it; these are objects of immediate experience, and may be correlated without raising the epistemological question as to the relation

between content of consciousness and changes in a material substratum. In these cases there is properly no question of psychophysical causation, but only the question of the possibility of an exclusively psychical causation as distinguished from the physical causation which we ordinarily invoke. Referring 'states of consciousness' to the concomitant 'bodily processes' may be a psychophysical procedure, or it may not: it depends on whether you refer to the phenomenal body or to the material body, and on how you define 'physical.'

Of course it would be unjust to charge Dr. Yerkes with responsibility for this psychophysical muddle, for he has simply introduced it by quotation from the standard authors who have muddled most thoroughly. My only complaint against Dr. Yerkes is that he did not make clear his own position in regard to the distinction between the physical and the psychological, and in regard to the distinction between the mind and the body. It is impossible to read his book without wondering what his criterion actually is.

In an article which preceded the *Introduction*,[1] and which seems to contain the same theory of causality which is embodied in the later volume, Dr. Yerkes has defined the physical as dealing with things from the "objective point of regard or attitude," and the psychical as dealing with things from the "subjective attitude," "as existent in the consciousness of the observer." This method of definition does no more than bring in two more ambiguous terms, for the distinction between 'subjective' and 'objective' is quite as vague as the distinction between 'mind' and 'body.'

In the attempt to make my meaning clear, I may point out that a way of distinguishing between the physical and the non-physical (which includes, and perhaps is exhausted by, the 'psychical'), might be based on the notion of causal explanation, or rather on a particular notion of causal explanation. I might claim that whenever we attempt to explain causally, or to deal with things in a way which implies a logical causal principle (matter, in the strict sense), we are dealing physically. When, on the other hand, we deal with things in a way which does not explain or imply a causal principle, we are dealing psychically (or at least non-physically). The acceptance of this principle, which I am not defending, would be a real step towards definiteness in the discrimination of the physical from the non-physical. Dr. Yerkes explicitly repudiates this principle. It is not clear that he adopts any other.

KNIGHT DUNLAP

THE JOHNS HOPKINS UNIVERSITY

[1] *Jour. of Philos., Psychol., etc.*, 1910, 7, 113–124.

A COMMUNICATION

MONOCHROMATIC LIGHT AND VISUAL ACUITY

Owing to the fact that the human eye is not achromatic, monochromatic light should produce a more distinct retinal image than light having an extended spectrum. The superior defining power of monochromatic light was shown in the following manner and published in detail in the *Electrical World*, Aug. 19, 1911.

The green line of the mercury vapor spectrum was isolated and used as a monochromatic source. This line was matched in hue by light having an extended spectrum obtained by filters used with the tungsten lamp. These two green colors could easily be matched for brightness without any of the difficulties which would obtain if they differed in hue.

A printed page of type of such size that at a distance of 1 meter it was just readable in monochromatic light, was arranged in a photometer so that two adjacent patches were illuminated respectively by the two green lights of different spectral character. For the same ease in reading it was found that the illumination having an extended spectrum must be increased 75 per cent. over the monochromatic illumination. This result was substantiated by other experiments. Later the Ives[1] acuity test object was used and by this more sensitive method it was found that for two observers the illumination having an extended spectrum must be five times greater than that of the monochromatic illumination for the same visual acuity. Another observer required an increase of only 33 per cent. It was shown that there was no movement of the pupil when alternately subjected to the two lights. Other experiments were made which bore on the subject of the use of the acuity method in the measurement of illumination. Photographs of the transmission of the filters used and a table of results are given in the original article.

M. LUCKIESH

PHYSICAL LABORATORY,
NATIONAL ELECTRIC LAMP ASSOCIATION,
CLEVELAND, OHIO

[1] *Elec. World*, April 14, 1910.

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SEVERIN, H. H. P., and SEVERIN, H. C. *An Experimental Study on the Death-Feigning of Belostoma* (= *Zaitha Aucct.*) *flumineum Say and Nepa apiculata Uhler.* (Behavior Monographs, 1, No. 3.) New York: Holt, 1911. Pp. iii + 44.

NOTES AND NEWS

THE meeting of the American Psychological Association will be held in Washington, December 27 to 29 inclusive, under the presidency of Professor Seashore. Joint sessions are being arranged with Section L of the American Association for the Advancement of Science and with the Southern Society for Philosophy and Psychology. This latter will be held at the Government Hospital for the Insane, at the invitation of Dr. Franz. It is probable that a sectional meeting devoted to papers on animal behavior will be held jointly with Section F. Arrangements have already been completed for a symposium upon the demarcation of the concepts, instinct and intelligence. The official headquarters of the Association will be at the Ebbitt House.

DR. F. LYMAN WELLS has severed his connection with Ward's Island and has returned to his former position at McLean Hospital.

THE following items are taken from the press:

AT Cornell University, Dr. Guy M. Whipple has been advanced to an assistant professorship of the higher grade, with the designation 'educational psychology' in place of 'the science and art of education.' Professor Whipple has assumed the editorial direction of the series of Educational Psychology Monographs, published in connection with the *Journal of Educational Psychology*.

DR. L. R. GEISSLER, of Cornell University, has been appointed associate psychologist in the physical laboratory of the National Electric Lamp Association, Cleveland, O.

AT Northwestern University, Robert Harvey Gault, Ph.D., has been advanced from an instructorship to an assistant professorship in psychology and has been appointed editor of the *American Journal of Criminology*.

ALFRED BINET, director of the Psychological Laboratory of the University of Paris and editor of *L'Année psychologique*, died in Paris, on October 19, at the age of fifty-four.

DR. JOHN HUGHLINGS JACKSON, the 'father of English Neurology,' died in London, on October 7, at the age of seventy-six.